

Royds rarely rises above freezing-point, and there is no vegetation higher than mosses. It is therefore surprising to hear of an abundant microscopic fauna and flora. Mr. Murray's experience stood him in good stead, for he made much of a very unpromising centre of operations. "The kinds of animals which are usually to be found among mosses have at Cape Royds a shelter of another sort, which, judging from their numbers, appears to suit them better. This is furnished by the foliaceous vegetation which grows so abundantly in the lakes and ponds." Thus Mr. Murray reports:—"I have never anywhere seen bdelloid rotifers so plentiful as are the two dominant species at Cape Royds (*Philodina gregaria* and *Adineta grandis*). . . . The water-bears are of only a few kinds, but one of them (*Macrobiotus arcticus*) is extremely abundant. There are nematode worms of two or more kinds, mites of several kinds, and two crustacea belonging to the Entomostraca. The ciliate infusoria are very numerous, there are a good many flagellata, but only two rhizopods were observed." Numerous microphotographs were taken under disadvantageous conditions, and some of these are printed—showing not only rotifers, water-bears, and the like, but some other creatures which the editor has wisely refrained from naming.

Some sixteen species of rotifers were distinguished, representing all the orders, though mostly bdelloids. This is the first definite record of rotifers within the Antarctic Circle, and five of the bdelloids are new species. The most interesting facts are those regarding the toughness of the rotifer constitution. Thus *Philodina gregaria* n. sp. is normally frozen in the ice of the lakes for the greater part of the year, and revives at any time that the ice is thawed. It may be alternately thawed and re-frozen at weekly intervals for several months. In England it was subjected to a temperature of -78° C. for many hours, by Mr. J. H. Priestley, of Bristol, and survived. Of *Adineta grandis* n. sp., which survived the lowest temperatures experienced at Cape Royds (-40° F.), and repeated freezing and thawing, and immersion for a month in sea-water, it is further recorded that "a proportion of them lived after the bottle containing them (in the dry condition) was immersed in boiling water for a short time. It was one of the rotifers which was to be seen alive and active in London in September, 1909, after being dry for about a year, and spending some months in tropical and sub-tropical climates." This toughness of constitution is interesting in several ways; e.g., in showing that these Antarctic rotifers can stand very adverse circumstances in the course of dispersal. Another point of interest concerning the rotifers is that the two dominant species, named above, are viviparous, which seems therefore the mode of reproduction best adapted to secure success in the struggle of existence under the severe conditions at Cape Royds. M. Jules Cardot reports on four mosses; the rest of the report is due to Mr. Murray, to whom we offer congratulations.

(3) From a third Antarctic expedition, the *Belgica* (1897-9), some additional reports have been recently received. Thus Dr. H. J. Hansen, who has done such good work among the crustacea, describes some new schizopods and cumacea. He indicates, as other authorities on crustaceans have done, that the familiar title schizopods will have to go, and that the orders of Euphausiacea and Mysidacea, which it includes, are far from closely related to each other. In regard to *Euphausia superba* he notes that it is the staple food supply of seals, such as *Lobodon carcinophaga*, and that it seems to live everywhere in the Antarctic Ocean. A. Pelikan gives a petrographical account of diorites,

gabbros, porphyrites, and other types of rock collected by the expedition. Prof. A. Gilkinet reports on a few fossil plants from Magellan; *Fagus*, *Nothofagus*, *Myrtiphyllum*, *Saxegothopsis*, which seem to be of relatively recent age, and bear a close resemblance to members of the present-day flora of that region. H. van Heurck reports on the diatoms and adds to the value of his work by a survey of polar diatoms in general. Henryk Arctowski gives a beautifully illustrated account of his personal observations on the different kinds of ice and their transformations. These observations are all the more interesting since very little was known of southern ice before the voyage of the *Belgica*. As the author indicates, a good deal has been done since.

ATOMIC WEIGHTS.¹

DR. CLARKE continues to put all chemists under an obligation to him by reason of the zeal and care with which he collects and disseminates information concerning the most important of all chemical constants—the atomic weights of the elements. In the volume before us—the third edition of a work with which his name is inseparably associated—he has brought to a focus all contemporary knowledge on the subject, discussing, digesting, and weighing the experimental evidence with the same lucidity, completeness, and impartiality which have characterised his previous publications.

It is interesting and instructive to compare the present issue with the original one of 1882. The number of the chemical elements has not greatly increased during the last thirty years. Even including the inert gases of the atmosphere and such of the radio-active elements of which the individuality may be said to be established, the increase is not more than about a dozen, and such values of their atomic weights as we possess are only of the order of first approximations. The most significant feature of the difference between the two issues is seen rather in the far higher standard of accuracy which is now required in such estimations. It is absolutely useless nowadays for anybody to engage in such determinations who is not prepared to impose upon himself the most rigorous checks, the most scrupulous attention to detail, and an inflexible determination to put forward no result that will not stand the severest scrutiny.

Atomic weights to-day are required for other purposes than chemical arithmetic, and comparatively rough approximations serve for the greater number of the operations of quantitative analysis. The errors due to manipulation, and to the use of methods faulty in principle, are, as a rule, far larger than those due to the employment of incorrect values for the atomic weights. Instances, indeed, might be quoted where it is apparently necessary to adopt a confessedly inaccurate value for an atomic weight in order to compensate for the error due to an imperfect method of quantitative estimation. Certain large trade operations could not be equitably arranged on any other basis. This, of course, does not concern the chemist as a man of science, and is certainly no argument for the retention of an inaccurate constant in our tables.

¹ (1) The Constants of Nature. Part v., A Recalculation of the Atomic Weights. Third Edition. Revised and Enlarged. By Frank Wigglesworth Clarke. Pp. iv+548. (Washington: Smithsonian Institution, 1910.)

(2) Determinations of Atomic Weights. Further Investigation concerning the Atomic Weights of Silver, Lithium and Chlorine. By Theodore W. Richards and Hobart Hurd Willard.

The Harvard Determinations of Atomic Weights between 1870 and 1910. By Theodore W. Richards.

Methods used in Precise Chemical Investigation. By Theodore W. Richards. Pp. iv+113. (Washington: Carnegie Institution, 1910.)

of atomic weights. These require to be known with all the precision of which quantitative chemistry is capable.

It is only by such knowledge that we may hope to find solutions to some of the most interesting and important problems with which contemporary chemistry is confronted. There is, to begin with, the fundamental question of the validity of the law of the conservation of mass. Is there a dissipation of matter, as of energy, in a cycle of chemical changes? Is an atomic weight a fixed and unalterable quantity? Or is it, as first suggested by Marignac, a statistical quantity varying within limits, doubtless very small, but still possibly appreciable?

There is further the perennial question of Prout's law, which, like the poor, seems to be always with us. Modern views of the genetic relations of the elements and of the dependence of their properties upon their relative masses are intimately connected with the exact values and numerical relations of atomic weights. It is these and similar questions lying at the very basis of chemical philosophy that render it imperatively necessary that these constants should be known with the greatest possible precision. The greatest possible precision is, of course, relative; it depends upon the degree of perfection of contemporary quantitative chemistry, and as this is progressive, each decade seeing improvements, both in the application of old methods and in the discovery of new, it necessarily follows that there is no such thing as finality in measurements of this kind. A large number of atomic weights are now known with accuracy to the first decimal place; even in the case of those of high values a considerable proportion indeed are known even to the second decimal, and a few, especially those elements which are habitually employed as a basis of comparison in atomic-weight work, as, for examples, silver and the halogens, are being ascertained with a still greater exactitude. It is only when atomic weights in general are known to a like degree of precision that we can hope for definite answers to such questions as have been indicated above.

It is largely due to the attention which this subject has received in America that our present position has been reached, and it is especially to the Harvard School of Chemistry that we are indebted for the high standard of accuracy which is now incumbent on every worker in this field of determinative chemistry. No laboratory in the world can point to such a remarkable sequence of memoirs as those which are embodied in the short synoptical statement in which Prof. Richards has dealt with the Harvard determinations of atomic weights between 1870 and the present year. Initiated by the late Prof. Josiah Parsons Cooke, whose determination of the atomic weight of antimony is still regarded as the best ascertained value for that element, the work has been continued by his assistant and successor, Prof. Theodore Richards, partly alone, but mainly in collaboration with pupils whom he has trained and imbued with his own high sense of exactitude. What the outcome of this work, extending over many years, has been is abundantly illustrated by the significant table on p. 90 of Prof. Richards's memoir. Of the eighty-three elements at present known, and of which the atomic weights are given in the annual tables prepared by the International Committee on Atomic Weights, no fewer than twenty-eight of those estimations which are regarded by the committee as among the best ascertained values are to be credited to the Harvard laboratory.

It is remarkable that this work should have been

done in America. It is commonly held that no nation is more keenly appreciative of the utilitarian value of science than America; but there is no money to be made out of the results of an atomic-weight determination. It is quite impossible to evolve a new colouring matter out of it, or to turn it into a synthetic drug. Not even the "smartest" and most enterprising of German chemists could bring it within the protective influence of the mystic letters D.R.P. On the contrary, atomic-weight work requires money, and that frequently in no small amount; platinum vessels, and apparatus of transparent quartz, electric ovens, high-class balances, and pure materials, render such work extremely costly. No doubt Harvard is well endowed, and Prof. Richards presumably has been liberally supported by his university. But the beneficence of the university has been largely supplemented by the action of the Carnegie Institution of Washington; without the pecuniary help afforded by the Trust, the work could not, says Prof. Richards, have been carried out on so large a scale, nor could it have reached the degree of precision which it has attained.

T. E. T.

TESTS FOR COLOUR VISION.

THE agitation concerning the official colour-vision tests for seamen has entered upon a new stage. The Board of Trade has announced its decision to hold an inquiry into the matter, and the *personnel* of the committee has been published (*NATURE*, June 30, p. 529).

After the reiteration of the confidence of the Board in the certitude of the official tests, this change of front comes somewhat as a surprise, but that can be forgiven in the welcome possibilities of a revision of tests that have forfeited the confidence of those most concerned.

It is not to be expected that the constitution of an official committee will please everyone, and already protest has been made by letter to the Board of Trade from the secretary of the Imperial Merchant Service Guild. The Guild protests that it was given to understand that the projected new committee would be of small size and of strictly impartial character, but that it proves to be large, and in the view of the Guild heavily weighted. The Guild states that of the members of the committee, at least two were prominent supporters of the official tests in recently disputed cases where the official position was admittedly wrong.

The choice of tests for colour vision is not so simple as it may seem at first sight. The difficulties presented in appreciating the mental picture of a colour-blind person are very great; and complexity is introduced by the several conflicting theories of colour vision which, consciously or unconsciously, bias the opinion of those who essay to determine these tests.

It is perhaps unfortunate that theories of colour vision should enter into the question of colour tests, at any rate, in the present state of our knowledge. It is, of course, conceivable, nay, even probable, that the true theory of colour vision when that is formulated and proven, supposing it for the moment to be none of those extant, will show an infallible means of testing the sense of colour in any and every person. Until then it would appear better that the test should be frankly empirical, and, so far as possible, unbiased by any theory. And for the reason that, unless rival theories be eliminated from the field, we can scarcely expect a reasonable uniformity in our tests.